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Some of these outstanding terms are found to correspond closely in both period and coefficient with those known to exist and to be absent from Hansen's Tables, and the fact that the values so determined correspond so closely with the values furnished by theory furnishes the strongest evidence of the reality of the existence of the other similar terms whose values are not so readily determined from theory.

6. The full details of the results of the reduction and discussion of this great mass of observations as compared with Hansen's Tables, has been lying for some time at the Natal Observatory awaiting publication. For it was decided by the Natal Government that as the work represented a considerable part of the scientific work done at the Colonial Observatory, it must not be published in any other form than as part of the publications of the colony. Up to the present no provision has been made to meet the cost of printing, but in the estimates for the coming year, it is trusted, provision will be made for the due printing and publication of this investigation. If so, they will appear in the course of the coming year, and the enormous mass of work be available for general use.

7. With regard to the theoretical calculation of these inequalities it is not necessary to say anything, as my views are well known. Most of those which require consideration have been calculated by two independent methods which have been already described in the *Monthly Notices* of the Society. So far the final reduction and numerical calculation has been suspended, so that the values deduced from the observations shall remain absolutely independent. The theoretical determination of these terms will form, therefore, a subsequent volume of the publications of the Natal Observatory as soon as the necessary funds for printing are available; and as it will be a heavy mass of mathematical printing, its cost will be much heavier than the portion of the complete whole formed by the reduction and discussion of the Greenwich lunar observations of the last 150 years.

Natal Observatory:
1903 *May* 9.

Note on the use of Peirce's Criterion for the Rejection of Doubtful Observations. By S. A. Saunder, M.A.

It would seem at first sight that when for observations made at the telescope we substitute a series of photographic measures we ought to be able to eliminate all those abnormal errors or real mistakes, such as the entry of a wrong figure, which are not con-

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templated in the Theory of Errors, but which undoubtedly do occur in practice, and which have led to the suggestion of various criteria for the detection and consequent rejection of such abnormal observations.

With regard to such classes of mistakes as may be supposed to have most frequently occurred in the case of telescopic observations, this is undoubtedly the case, for a photographic measure which gives an abnormal result, and so comes under suspicion, may be repeated any number of times, and any clerical error or mistake in reading may be certainly detected and eliminated.

But a new possibility of error is introduced at the same time. A plate is either accepted or rejected as a whole in accordance with the general excellence or the reverse of its definition. But it may well happen that a plate which gives excellent definition as a whole may yet have some defective images. How far this is likely to occur in star photographs, such as those taken for the astrographic catalogue, I have no experience; but it does, and I think must, happen in every photograph of the Moon. Formations near the terminator may be beautifully sharp, whilst from unsuitable illumination, or from the general brightness of a district, those in other parts may be quite unmeasurable. Between these two extremes there will be an almost continuous gradation, and it becomes an anxious question where to draw the line between those formations which are to be measured and those to be rejected. Another possible cause of error may be that the assumed positions of some of the standard points are seriously wrong. Where these standard points are stars which have been repeatedly observed with meridian instruments such errors are not likely to be great; but in an investigation such as a survey of the Moon, where in our present state of knowledge the standard points have to be built up as we proceed, it is a possibility not to be lost sight of. There is too the, probably small, risk of distortion of the film.

In a recent difficulty, due to an attempt to measure two badly defined formations, I found Peirce's criterion so useful that I thought it might be of interest to call attention to it, especially as I believe that the criterion is seldom if ever employed by the most experienced computers, at all events in this country.

The plate in question was one of those most kindly placed at my disposal by M. Loewy; it was taken at the Paris Observatory on 1899, March 22, and shows something like three quarters of the disc under morning illumination with a terminator at about +41°. Some 300 or 400 points have been measured by Mr. Hardcastle on this plate with considerable certainty, but most of them are near the terminator, and in order to determine the constants of the plate it was very desirable to have some points near the west limb. Two of the best determined of such points are *Macrobius a* and *Proclus*, whose positions have been measured 11 and 12 times respectively by Dr. Franz with the Königsberg

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heliometer.* The formations were both over exposed on the plate, and Mr. Hardcastle noted at the time he made the measures that considerable uncertainty attached to them. The value of such points is, however, so great for determining the scale and orientation that I decided to include them in a group of 45 points, giving 90 conditional equations, from which the four constants of the plate were to be determined.

When the solution was completed it was found that *Macrobius a* gave a residual of 65 in ξ, *Proclus* one of 68 in η; the unit being ·00001 of the Moon's radius, so that each residual was about 0″·6. These were the greatest residuals found; they were so large as to make me uneasy about them, and yet not so large that I could unhesitatingly say that they ought to be rejected. In this dilemma I computed the value of Peirce's criterion, which gave 63 as the limit for rejection of a single observation, the mean error of a co-ordinate being 22·9. The critical value for a third observation was 54, and the next residual actually occurring was 50. The criterion thus gave me just the additional assistance I required in order to come to a decision; I rejected the two points and solved again. The mean error fell to 20·1, the value of the Moon's radius, giving the scale of the plate, was altered by 0″·06 with a probable error ± 0″·043, the orientation was altered by 33″ with a probable error ± 9″·7, the discordances in the positions of the two rejected points each rose to 74, and the residual of 50 fell to 46. The general impression left on my mind was that the solution was much improved.

The general question of the rejection of discordant observations has been discussed by Dr. Glaisher,† who strongly upholds De Morgan's view that when recognisable mistakes have been eliminated no observation should be rejected, but that when the first solution has been effected the conditional equations should be re-weighted in accordance with their residuals and solved again, the process being continued until the weights assumed for the last solution are sufficiently near to those given by that solution. How many solutions this would in general require I do not know, but it is clear that the process might be a very laborious one. In many cases its length would be practically prohibitive, and if we decide to give all the equations, either unit or zero weight, it is certain, as Dr. Glaisher points out, that we shall get a better result by excluding certain discordant observations than we should by including them.

I believe that the practice amongst computers of experience is to rely almost entirely on their individual judgment, taking into account the conditions of the observations, and drawing the line somewhere about those observations which give residuals of five times the probable error. But for those who, like myself, have a very limited practical acquaintance with least square

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* *Königsberg Beobach.*, Band 38; 'Die Fig. des Mondes,' pp. 10, 11.

† *Monthly Notices*, Vol. xxxiii., pp. 391–402.

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solutions, difficult cases, like the one I have just quoted, must from time to time arise, and I cannot help thinking that others may be as glad as I have been to have recourse to a criterion established on some definite principle, which may be considered as an embodiment of that accumulated experience which we lack.

The computation of the criterion is quite a short matter with the help of such tables as those given in Chauvenet, but it is only to be effected through successive approximations by trial and error, and in order to facilitate its application I have computed a short table which I append to this paper.

If ε denote the mean error (error of mean square) as given by the least square solution, the critical residual may be denoted by $\kappa\varepsilon$, where κ is the quantity it was the object of Professor Peirce's investigation to discover. Values of κ^2 are given in Chauvenet, as computed by Gould, for any number of conditional equations up to 60 involving either one or two unknowns. But in determining the constants of a photographic plate the number of unknowns which enter into a group of conditional equations is usually either three or four. I have accordingly computed the values of κ for these cases and for numbers of conditional equations between 10 and 100.

It did not seem necessary to make the table as elaborate as Dr. Gould's, since interpolation is easy, and the values given are, even theoretically, only approximate. The theoretical value depends upon the ratio of the two values of ε when the questionable observations are included, and when they are excluded; this ratio must vary in different cases, but an approximation is made by assuming that the sum of the squares of the residuals is diminished only by the squares of those given by the observations rejected. This gives the new value of ε a little too large and consequently makes κ a little too small. But as we are in the neighbourhood of a minimum value of the sum of the squares the difference is usually small.

Also it must be remembered that the criterion is to be considered only as a guide to the computer on whose judgment the final decision must ultimately depend.

In the following table

m denotes the number of conditional equations;

μ the number of unknown quantities;

n the number of observations it is proposed to reject.

It must be particularly borne in mind that the criterion for $n = 2$ is not to be applied unless one observation has been rejected by that for $n = 1$; that for $n = 3$, not unless two have been rejected by that for $n = 2$, and so on.

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Peirce's Criterion.

Values of κ.

$\mu = 3$.

m	n			
	1	2	3	4
10	1·72	1·44	1·28	1·16
12	1·83	1·55	1·38	1·26
14	1·92	1·64	1·47	1·35
16	2·00	1·72	1·55	1·42
18	2·06	1·78	1·61	1·48
20	2·12	1·84	1·67	1·54
25	2·23	1·96	1·79	1·66
30	2·32	2·05	1·88	1·75
35	2·39	2·12	1·95	1·83
40	2·45	2·19	2·02	1·90
45	2·51	2·24	2·08	1·95
50	2·55	2·29	2·13	2·00
60	2·63	2·37	2·21	2·09
70	2·69	2·44	2·28	2·16
80	2·74	2·49	2·34	2·22
90	2·79	2·54	2·39	2·27
100	2·83	2·58	2·43	2·32

$\mu = 4$.

m	n			
	1	2	3	4
10	1·63	1·37	1·22	1·11
12	1·76	1·49	1·33	1·22
14	1·86	1·59	1·43	1·31
16	1·94	1·67	1·50	1·38
18	2·01	1·74	1·57	1·45
20	2·07	1·80	1·63	1·51
25	2·19	1·92	1·76	1·63
30	2·29	2·02	1·85	1·73
35	2·36	2·10	1·93	1·81
40	2·43	2·16	2·00	1·88
45	2·48	2·22	2·06	1·94
50	2·53	2·27	2·11	1·99
60	2·61	2·35	2·19	2·08
70	2·67	2·42	2·26	2·15
80	2·73	2·48	2·32	2·21
90	2·77	2·53	2·37	2·26
100	2·81	2·57	2·42	2·31

On Oscillating Satellites. By H. C. Plummer, M.A.

1. In his paper "On Periodic Orbits"[*] (to be quoted here as *Ch.*) Professor Charlier has tried to obtain in a simple analytical manner some of the results already found by Professor Darwin in his well-known memoir[†] on this subject (to be quoted as *D.*). The orbits investigated by Professor Charlier are those of oscillating satellites—*i.e.* those small periodic orbits which can be described in the neighbourhood of the five points where exact solutions of the problem of three bodies are known to exist. Some of his results I have tried to discuss in a more general way in a paper[‡] (to be quoted as *Pl.*) in which I have considered the motion of a small body in the neighbourhood of centres of libration, or points of zero force, in any rotating field of force. In the present paper it is sought to push the analysis beyond the

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[*] *Meddelanden från Lunds Astronomiska Observatorium*, No. 18.

[†] *Acta Mathematica*, t. xxi. p. 99.

[‡] *Monthly Notices*, vol. lxii. p. 6.